Credit Market Conditions and Mental Health

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Abstract

Research offers conflicting predictions about the impact of credit conditions on mental health. We first assess how bank regulatory reforms that improved credit conditions, e.g., by enhancing the efficiency of credit allocation and lowering lending rates, impacted mental health. We discover that among low-income individuals, these regulatory reforms reduced mental depression, boosted labor market outcomes, eased access to mortgage debt, and reduced the ranks of the "unbanked." We also find that mergers of large regional banks that led to branch closures and tighter credit constraints in affected counties harmed the mental health of lower-income individuals in treated counties.

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1. Introduction

Research suggests that competitive, efficient banking systems spur economic growth. For example, Jayaratne and Strahan (1996, 1998) discovered that U.S. regulatory reforms that intensified bank competition improved the efficiency of credit allocation and lowered lending rates, accelerating economic growth.[1] However, banking systems can influence the human condition and overall utility beyond their effects on aggregate growth, raising questions about the impact of banks on overall welfare. We contribute to this research by assessing how credit conditions affect mental health.

Existing studies suggest how credit conditions could affect mental health positively and negatively. First, research shows that regulatory reforms that improve credit allocation and lower interest rates boost labor demand (Beck, Levine, and Levkov 2010). Research further documents a robust, positive relationship between labor market outcomes and mental health (e.g., Lund et al. 2010; Reeves et al. 2012; Allen et al. 2014; Ribeiro et al. 2017; Wickham et al. 2017; Patel et al. 2018; Persson and Rossin-Slater 2018; Christian et al. 2019; McGuire et al. 2022). Thus, regulatory reforms that enhance credit conditions could improve mental health by boosting labor market outcomes. Second, researchers find that regulatory reforms that intensify bank competition ease household credit constraints (e.g., Favara and Imbs 2015; Sun and Yannelis 2016; Agarwal et al. 2018; Célerier and Matray 2019; Mian, Sufi, and Verner 2020), which can enhance mental health by making it less costly for households to smooth shocks, make large purchases (e.g., Allen et al. 2014).[2] Third, research also indicates that regulatory reforms that improve bank efficiency tend to increase the ability of "unbanked" individuals to access banking services (e.g., Célerier and Matray 2019), with potentially positive effects on wellbeing (e.g., Agarwal, Ghosh, Li, and Ruan 2022). Research also indicates how improving credit conditions could harm mental health. For example, Dick and Lehnert (2010) find that regulatory reforms that ease household credit constraints increase bankruptcy rates among households previously excluded from bank credit, potentially harming mental health (e.g., Gathergood 2012).[3]

[1] Cross-country studies also indicate that bank development accelerates growth (e.g., Levine 1997; 2005).
[2] Furthermore, Bernstein and Koudijs (2023) show that the amortization of mortgage debt accounts for a significant portion of U.S. household wealth accumulation. Also, see, Brown et al. (2019) and D'Acunto et al. (2019).
[3] In addition, Kerr and Nanda (2009) show that enhancing credit conditions allow new, efficient firms to emerge and grow and force less efficient incumbents to shrink and close. The resultant increase in labor market instability could adversely affect mental health (e.g., Metcalfe et al. 2003). Favara and Imbs (2015) find that easing household credit constraints increases the demand for

We employ two empirical strategies to assess the impact of credit conditions on mental health. Our first strategy exploits a quasi-experimental shock to credit conditions: the deregulation of restrictions on interstate bank branching that began in 1995 and continued through 2005. Although the Riegle-Neal Act eliminated regulatory prohibitions on interstate banking in 1995, it did not prohibit states from erecting barriers to out-of-state branch expansion. Since branching is less expensive than establishing subsidiaries, interstate branching restrictions limited competition between banks in different states and made banking markets less efficient. Rice and Strahan (2010) show that lowering regulatory barriers to interstate branching intensified competition and improved credit conditions by reducing the interest rates charged by banks on loans. Building on their strategy, we use cross-state, cross-time variation in the removal of regulatory impediments to interstate bank branching as an exogenous source of variation in local bank competition and examine the impact of these regulatory reforms on mental health. We also extend their analyses to reduce identification concerns that other factors drive both deregulation and mental health.

We explore the impact of interstate branch deregulation on mental illness using data from the National Longitudinal Survey of Youth 1979 (NLSY79). The NLSY79 is a nationally representative survey that follows 12,686 individuals born between 1957-1964 from 1979 to the present. Besides information on employment, income, and demographics, the NLSY79 obtains information on each individual's mental health. The NLSY79 creates a *Depression Index* based on the Center for Epidemiological Studies Depression Scale, and we use this index in our study. Critically, the NLSY79 provides the *Depression Index* for each individual over time. Thus, we test whether bank deregulation triggered changes in individuals' mental health.

In assessing individuals' mental health responses to branch deregulation, we differentiate between high- and low-income individuals. We differentiate by income because past research suggests that bank regulatory reforms that improved firm and household credit conditions disproportionately influenced lower-income workers. For example, research shows that U.S. regulatory reforms that intensified bank competition disproportionately boosted the demand for

housing, boosting home prices. Mason et al. (2013) show that declines in housing affordability reduce mental health, illustrating another mechanism through which enhancing credit conditions could harm mental health.

lower-income workers (e.g., Beck, Demirgüç-Kunt, and Levine 2007; Demirgüç-Kunt and Levine 2009; Beck, Levine, and Levkov 2010). Research also indicates that these regulatory reforms primarily eased household borrowing constraints on households previously excluded from bank credit (e.g., Dick and Lehnert 2010). Since excluded households tend to have lower incomes (e.g., Campbell 2006), this work suggests that deregulation disproportionately affected lower-income households. Thus, we assess the differential impact of branch deregulation on the mental health of higher- and lower-income households.

We discover that (1) interstate branch deregulation is associated with a significant drop in mental depression, and (2) improvements in the mental health of low-income individuals drive this effect. We use a control function approach to reduce omitted variable concerns by controlling for an array of factors, including individual fixed effects, gender-race-year fixed effects, state-specific linear time trends, dummy variables for the year and month of the NLSY79 survey, and time-varying state characteristics. By controlling for individual and gender-race-year effects, we condition out time-invariant individual-level characteristics that shape mental health and time-varying factors that affect these sub-population groups differently. By including state-specific linear time trends and time-varying state traits, we reduce concerns that omitted state-specific factors account for the findings. By including survey year and month fixed effects, we control for the possibility that season factors around the dates of each survey influence the results. On identification, we also show that the pre-treatment parallel trends assumption holds: before deregulation, low-income individuals in states that ultimately deregulate more have almost the same trends in mental depression as those that eventually deregulate less; then, there is a notable improvement in mental health only among low-income individuals living in states that deregulate more.

The estimated impact is large. Suppose the average state relaxes interstate branching restrictions by one degree—where branch restrictions range from zero (most restrictive) to four (least restrictive). In that case, the estimates indicate that the composite depression index (*Depression Index*) for the average low-income individual drops by 14%. To further assess the impact, we conduct the analyses on an alternative mental health indicator, *Depression Index ≥ 16*, which equals one if the *Depression Index* is greater than or equal to 16, a cut-off value used by physicians to detect major depression syndrome and disorder (Juarros-Basterretxea et al. 2021). Using this binary measure of mental health, our estimates indicate that lowering interstate branching restrictions by one degree reduces the incidence of major depression syndrome and disorder among low-income individuals by two percentage points. This is a large effect, as the average value of *the Depression Index* $\geq$ *16* in the sample of low-income workers is 3.8%.

We conduct two additional robustness checks. First, although we control for an array of fixed effects and time-varying state characteristics, we also employ a spatial regression discontinuity design to enhance identification. In particular, Huang (2008) compares the economic performance of neighboring counties separated by state borders when one state deregulates interstate banking. Huang (2008) argues that neighboring counties likely have similar observable and unobservable traits, reducing omitted variable concerns and enhancing identification. We apply this method to interstate branch regulation: we compare two counties separated by a state border and evaluate the impact of changing interstate branch regulations in one county on the comparative mental health metrics in those counties. Using this strategy, we confirm that interstate branch deregulation improves mental health among low-income individuals.

Second, we address potential concerns that staggered difference-in-differences (DID) regressions can yield biased estimates (e.g., de Chaisemartin and D'Haultfoeuille 2020; 2023; Borusyak, Jaravel, and Spiess 2021; and Baker et al. 2022). The biases can occur when DID regression compares a later-treated group to an earlier-treated group while treating the earlier-treated group as a control group. This bias is unlikely in our setting, because we only observe one treated observation for mental health, except for a few individuals in Alaska. We find that all results hold when excluding Alaska or using the Borusyak, Jaravel, and Spiess (2021) correction for staggered DID regressions.

We extend these analyses by exploring three channels through which branch deregulation may affect mental health. Bank deregulation can exert multifaceted influences on the economic and financial conditions shaping the lives of individuals. Although we do not seek to identify one, and only one channel, connecting branch deregulation and mental health, we can provide exploratory evidence on the labor market, household credit, and non-credit banking service channels. As noted above, the labor market channel holds that branch deregulation eased firm credit constraints and boosted the demand for labor, especially for low-income workers, with positive repercussions on mental health. Consistent with the labor market channel, interstate branch deregulation boosted labor market outcomes (employment and earnings) among low-income individuals but not high-income individuals. The household credit channel holds that branch deregulation eased household credit constraints, especially those constraining lower-income families, allowing those households to purchase homes and more easily smooth shocks, boosting mental health. Consistent with this channel, deregulation increased mortgage debt among low-income individuals but not high-income workers. The non-credit banking services channel holds that interstate bank branch deregulation improved mental health by expanding access to non-credit banking services among low-income individuals. Consistent with this view, interstate branch deregulation boosted access to saving accounts among low-income households but not among high-income individuals.

Next, we use a two-stage approach to provide additional information on the channels linking bank deregulation and mental health, though we acknowledge one potential limitation with these analyses. In the first stage, we use the interstate bank branch deregulation (IBBEA) index to predict the labor market outcomes, household credit, and access to savings accounts, respectively. We then run three separate second-stage regressions, one on each of the predicted channel outcomes. The results are consistent with the labor market view that interstate branch deregulation improved mental health by boosting the labor market outcomes of low-income individuals but inconsistent with the household credit and non-credit banking services channels. However, we view these instrumental variable (IV)-style analyses cautiously, as the validity of IV estimates rests on the assumption that the IV affects the dependent variable exclusively through its impact on the endogenous variable. Yet, our setting may violate this exclusion restriction, as we are assessing three potential channels, i.e., endogenous variables, through which our one instrument could influence mental health.

Finally, we extend the branch deregulation analyses by examining how the relationship between bank deregulation and mental health differs by household and community characteristics. We first consider the possibility that the impact of branch deregulation on mental health depends on pre-treatment household leverage. While deregulation could improve welfare by enhancing access to credit, it (1) would likely have smaller beneficial effects among those who were already highly levered and (2) could even have harmful effects on highly-levered individuals by facilitating overborrowing (e.g., Karlan and Zinman 2020, and Andersen et al. 2022). Consistent with this view, we discover that (1) among those with high leverage ratios, deregulation does not improve mental health and may even harm mental health at high enough debt levels, and (2) there is a robust negative relationship between bank deregulation and mental depression among low-income households that were not very highly levered before deregulation. Next, we consider the possible role of income inequality in shaping responses to deregulation. Suppose having a low income exerts a larger adverse impact on mental health among those living in high-income inequality communities, as Wilkinson (2002) suggested. In that case, we expect interstate branch deregulation to reduce depression more in high-income inequality states. This is what we find. Third, we consider political ideology's role in shaping deregulation's impact on mental health. If those on the political left value group-based income equality more positively than those on the right, as suggested by (e.g., Waldfogel et al. 2021; Goldman et al. 2021; Painter 2020), then deregulation would tend to improve mental more among those who care more about relative income: low-income, democratic households. Consistent with this view, we find a stronger relationship between interstate branch deregulation and mental health among lower-income households living in more Democratic states.

Our second empirical strategy exploits a different quasi-experimental research design in which there is an adverse shock to local credit conditions and assesses the impact on mental health. We follow Nguyen (2019), who shows that consolidating local banking markets through the closing of branches tightened credit constraints on small, local businesses. This finding is consistent with research showing that small firms rely heavily on geographically close banks for credit, as geographic proximity helps reduce the informational frictions associated with lending to small firms. (e.g., Petersen and Rajan 1994; 2002; Berger and Udell 1995; Berger et al 2005; 2014; 2018). As an instrumental variable for closing branches in a locale, we follow Nguyen (2019) and use the mergers of large, multi-state banks that have branches in the local market. The identifying assumption is that large regional or national bank mergers are exogenous to the mental health conditions in which they have branches. We first confirm Nguyen's (2019) findings in our setting: merger-induced consolidations tightened county-level credit constraints on small businesses. We then assess the impact of these shocks on mental health. Given the period we have merger-induced consolidation data, we cannot use the NLSY79 mental health data. Thus, we turn to the U.S. Panel Study of Income Dynamics (PSID). Although the PSID does not pose the questions composing the Center for Epidemiological Studies Depression Scale, the PSID obtains valuable information on respondents' mental distress that we use in these analyses.

The merger-induced consolidation study of county-level bank competition confirms the earlier findings: local banking conditions materially influence mental health. Low-income individuals living in counties experiencing a merger-induced shock that increases local bank concentration experience an increase in mental distress relative to individuals residing in control counties. This second strategy is a valuable complement to the interstate bank branch deregulation approach because it exploits a different quasi-experimental shock to banking conditions and uses a different measure of mental health based on the PSID rather than the NLSY79, reducing identification and measurement concerns.

Our study contributes to research on a consequential topic that has received little attention from financial economists: mental health. According to the U.S. Department of Health and Human Services (2019), one in five U.S. adults experienced mental illness in 2018, and Insel (2008) estimates that mental illness lowers U.S. earnings by almost $200 billion per annum due to absenteeism and lower worker productivity (e.g., Bartel and Taubman 1986; Kessler 2012; and Chisholm et al. 2016). Globally, the World Health Organization (2011, 2018) reports that mental illness is the leading cause of lost working hours and estimates that the global per annum cost of mental illness is $2.5 trillion. In addition, while a growing body of empirical research explores how cognitive and non-cognitive traits influence credit market behavior (e.g., Kuhnen and Melzer 2018; Parise and Peijnenburg 2019; and Gong et al. 2023), we examine the impact of credit conditions on mental health. Our work offers new evidence that banking systems influence mental health by shaping the economic and financial conditions in which individuals and households work and live.

The remainder of the paper proceeds as follows. Section 2 discusses the Riegle-Neal Act and the details of interstate branch deregulation. Section 3 presents the data and variable definitions. Section 4 presents the core analyses using interstate bank branch deregulation. Section 5 explores the channels linking branch deregulation and mental health and how the relationship between bank deregulation and mental health differs by household and community characteristics. Section 6 presents the results using the quasi-experimental design that exploits merger-induced bank consolidations. Section 7 concludes.

2. Bank Branch Deregulation

The Riegle-Neal Interstate Banking and Branching Act (IBBEA) of 1994 effectively eliminated (a) regulatory impediments to banks expanding across state lines through the establishment of subsidiaries and (b) federal barriers to interstate branching. However, the Act allowed states to limit bank expansion across state lines by establishing branches. Interstate branching can occur by an out-of-state bank (a) acquiring an in-state bank and converting that bank into its branches, (b) purchasing the branches of an in-state bank, or (c) establishing new branches within a state ("de novo" branching).

Following Riegle-Neal, states used four types of regulatory restrictions on interstate branching. First, some states imposed minimum age restrictions concerning how long a target bank must exist before being acquired and consolidated into branches. These minimum age restrictions, which have a federally set maximum of five years, make cross-state banking more costly because they require that banks (a) purchase an entire older bank, which is more costly than opening a branch, or (b) open a new subsidiary and then wait until the minimum number of years before converting the subsidiary into a branch. Second, some states prohibited de novo interstate branching, which is often the least expensive method of interstate branching. Third, some states banned out-of-state banks from acquiring a single branch or incomplete portion of an in-state bank's network, creating an additional barrier to cross-state branching. Fourth, some states imposed limits on the percentage of insured deposits in a state that a single bank could hold, potentially impeding large banks headquartered in other states from purchasing multiple banks in a state.

We construct an index of the extent to which bank regulations permit interstate branching in a state, following the work of Rice and Strahan (2010). Specifically, *IBBEA Index* takes on values between zero and four, where one point is added to the index if the state (1) does not impose a minimum age restriction for acquisition, (2) allows de novo interstate branching, (3) permits interstate branching by acquiring a single branch; and (4) sets the deposit-cap no less than 30%. Thus, larger values of the *IBBEA Index* indicate a more deregulated interstate branching environment. We use this index to gauge post-1994 cross-state variations in restrictions on interstate branching.

3. Data and Variables

3.1. NLSY79

We extract individual-level data from the National Longitudinal Surveys Youth 1979 (NLSY79), administered by the U.S. Bureau of Labor Statistics. The NLSY79 is a nationally representative survey that follows a sample of American youth born between 1957-1964. The NLSY79 interviewed 12,686 individuals (aged 14 -22 years) in the initial survey year, 1979. The surveys were conducted annually from 1979 to 1994 and then biennially afterward. Following Altonji and Pierret (2001) and Levine and Rubinstein (2017), we drop individual-year observations with a missing state identifier and those not residing in one of the 50 states or the District of Columbia. We also drop observations with missing data on critical variables and individuals who change their residence state over the survey waves in which our corresponding dependent variables are observed. As shown below, the results hold when including individuals who change states. We use the sample weights provided by NLSY79.

The NLSY79 has two advantages over several other individual-level U.S. datasets. First, it is a representative, longitudinal survey. The long-panel nature of the NLSY79 allows us to control for individual fixed effects to identify better the impact of interstate branch deregulation on mental health. Second, besides containing information on standard demographic and economic traits, the NLSY79 also has information on psychological health, family background, and several cognitive and noncognitive traits. The NLSY79 also has a disadvantage: it examines one cohort through time. Below, we confirm the findings using the Panel Study of Income Dynamics that is not limited to a single cohort.

3.2. Depression Measures from the NLSY79

We use measures of each respondent's level of depression based on questions in the Attitudes, Health, and Health Module 40 & Over sections of the NLSY79. The NLSY79 surveys each individual's mental health in 1992, 1994, and the year immediately after the person becomes 40 years old (which occurs during the 1998-2005 period). Thus, we have three measures of each individual's mental health: two before the 1994 Riegle-Neal Act and one afterward. Given data on mental health and the index of restrictions on interstate bank branching, our sample period runs from 1992 through 2005.

We adopt the Center for Epidemiologic Studies Depression Scale, which measures symptoms of depression using seven questions. The CES-D scale is one of the most commonly used depression assessment instruments.[4] Respondents evaluate their degree of agreement (during the past week) with the following statements:

(1) I did not feel like eating; my appetite was poor (*Trouble Eating*),

(2) I had trouble keeping my mind on what I was doing (*Trouble Concentrating*),

(3) I feel depressed (*Feel Depressed*),

(4) I felt that everything I did was an effort (*Everything an Effort*),

(5) My sleep was restless (*Trouble sleeping*),

(6) I felt sad (*Feel Sad*), and

(7) I could not get "going" (*Lethargic*).

For each statement, respondents choose one of four possible responses: (a) Rarely/None of the time/1 Day, (b) Some/A little of the time/1-2 Days, (c) Occasionally/Moderate amount of the time/3-4 Days, and (d) Most/All of the times/5-7 Days. We assign ordinal values of 0 to 3 points corresponding to these four responses.

Besides the responses to these seven questions, the NLSY79 also reports a total score (*Depression Index*) that aggregates the answers to each question. Thus, an individual's

[4] See the list by the American Psychological Association: https://www.apa.org/depression-guideline/assessment.

Depression Index in a particular survey could range between 0 (the individual chooses 0 for all seven questions, least depressed) and 21 (the individual answers 3 for all questions, most depressed). This *Depression Index* is often employed in epidemiological and treatment studies to screen for depression. Juarros-Basterretxea et al. (2021) find that Depression Index values of 16 or above correctly capture 90% of major depression syndrome and disorder. In our analyses, we focus on the continuous measure, *Depression Index*. However, the results hold when using a dummy variable that equals one if the depression index is equal to or greater than 16 and zero otherwise, as discussed below and reported in Appendix Table OA2.[5]

3.3. Individual Controls

We control for several individual-level characteristics inspired by a growing literature on the "economics of happiness" and subjective well-being (e.g., Dolan, Peasgood, and White, 2008). *Education* equals the highest grade completed by the respondent, ranging from below high school (less than 12 years of schooling) to advanced graduate (over 16 years of schooling). *Age* equals the difference between the birth year and the survey year. Concerning family background, *Family Income* equals the natural logarithm of one plus the average total family income over the 1979 to 1981 period (in 2010 dollars). *Mother's Education* and *Father's Education* equal the maximum number of years of schooling received by the respondent's mother and father, respectively. *D-both Parents* is a dummy variable that equals one if the respondent lived with both parents at the age of 14 and zero otherwise.

We also condition on the following measures to capture an individual's skills, attitudes, and personality traits. *AFQT Ability Test* equals the respondent's percentile score on the Armed Services Vocational Aptitude Battery (ASVAB) test administered in 1980, where the test is designed to measure knowledge and skill in arithmetic reasoning, word knowledge, paragraph comprehension, and numerical operations. *Rosenberg Self-Esteem* equals the individual's score using the Rosenberg Self-Esteem Scale, computed from respondents' answers in 1987 to

[5] To reduce potential concerns with these mental health indicators, we conduct the following two assessments. First, we conduct a robustness check using data on suicides. We evaluate the impact of interstate branch deregulation on the growth of suicides across demographic groups (gender, race, and age group) in each state and year and confirm the paper's findings (Online Internet Appendix Table OA3). Second, as described below, we confirm the results using a different dataset that uses a different measure of mental health and a different shock to credit market conditions.

questions designed to measure self-worth (e.g., Rosenberg 1979).[6] *Pearlin Sense of Control* equals the Pearlin Mastery score based on questions from the 1992 survey and is based on answers to seven questions designed to measure the respondent's sense of control over one's life (e.g., Pearlin et al. 1981).[7] *D-Risk Attitude* is a dummy variable that equals one if the respondent answers "Yes" to the question: "Would you take a job that could either double family income or cut income by a third" in the 1993 survey year, and zero otherwise. We use *D-Risk Attitude* as a proxy for the respondent's level of risk tolerance just before the 1994 IBBEA deregulation.

3.4. State-Year Controls

In our analyses, we control for the following time-varying, state-specific traits (*State-Year Controls*) that are not part of the NLSY79 data: the growth rate of real per capita Gross State Product (*per-capita GSP Growth*), the natural logarithm of the population (*Log Population*), the percentage of the population living below the poverty line (*Poverty Rate*), the percentage of the population who are unemployment (*Unemployment Rate*), the percentage of the population in the labor force (*Labor Force Participation Rate*), and the natural logarithm of the residential house price index (*Log House Price Index*). We adjust all nominal values into 2010 prices using CPI prices.

3.5. Potential Channels

We also examine three channels through which interstate bank branch deregulation could influence mental health: labor market outcomes, households accessing credit, and households accessing non-credit banking services. Regarding labor market outcomes, we examine the (1) natural logarithm of one plus the total number of weeks the respondent is employed during the year (*Log Work Weeks (Annual)*), (2) the natural logarithm of one plus the total number of hours the respondent is employed during the year (*Log Work Hours (Annual)*), and (3) the natural

[6] Respondents were asked to choose (1) Strongly Agree, (2) Agree, (3) Disagree, and (4) Strongly Disagree over ten statements, such as "I am a person of worth," "I have a number of good qualities" etc. The total score ranges from 0 to 30, with a higher value indicating a higher level of self-esteem.

[7] Respondents were asked to choose (1) Strongly Disagree, (2) Disagree, (3) Agree, and (4) Strongly Agree over seven statements, such as "I have little control over the things that happen to me," "There is little I can do to change many of the important things in my life" etc. The total score ranges from 7 to 28, with a higher value indicating less subjective agency over one's future.

logarithm of one plus the respondent's real wage income (in 2010 thousand dollars) in the year (*Log Wage Income (Annual)*). We also examine labor market outcomes at the weekly frequency, using (1) a dummy variable that equals one if the respondent works during the week (*Is Working (Weekly)*) and (2) the natural logarithm of one plus the total number of hours the respondent works during the week (*Log Work Hours (Weekly)*). We do not have data on weekly earnings. Concerning households accessing credit, the NLSY79 provides annual data on three types of household debt. We examine each of them, all measured in 2010 thousand dollars: (1) the natural logarithm of one plus mortgage debt (*Log Mortgage Debt*), (2) the natural logarithm of one plus car loans (*Log Car Loans*), and (3) the natural logarithm of all other debts greater than $500 (*Log Other Debt > $500*).[8] Regarding households accessing non-credit forms of banking services, the NLSY79 asks respondents each year, "Do you [or your (husband/wife)] have any money assets like savings accounts?" If the respondent answers "yes" to this question, we code the variable *Has Savings Account* equal to one and set *Has Savings Account* equal to zero if the respondent responds "no" to the survey question.

3.6. Summary statistics

Table 1 provides summary statistics on critical variables using the sampling weights provided by the NLSY79 over our 1992-2005 sample period. Appendix 1 provides detailed variable definitions.[9] The *Depression Index* ranges from 0 (not depressed at all) to 21 (most depressed), and the sample average is 3.5. Among the low-income individuals in the sample, the average depression index is 4.3, and 3.8% have *Depression Index* values equal to or above sixteen.

Figure 1 graphs the average value of the *Depression Index* from 1992 through 2014. As shown, depression fell during the rapid economic growth of the 1990s and rose sharply following the 2008 global financial crisis. These aggregate movements are consistent with past work showing the positive association between economic conditions and mental health. In our analyses, we will focus on individual-level data and will condition out these aggregate trends.

[8] The NLSY79 survey questionnaires on household balance sheets changed substantially in 2004 and did not collect relevant data between 2000 and 2004. Thus, we use the sample period through 2000.
[9] For all continuous variables with unbounded tails, we winsorize at the 2.5th and 97.5th percentiles.

4. Bank Branch Deregulation and Mental Health

In this section, we exploit the cross-state, cross-time variation in bank deregulation and the longitudinal nature of the NLSY79 to examine the relationship between branch deregulation and mental health. We demonstrate that (a) the findings are robust to including or excluding an array of controls, including individual and gender-race-year fixed effects and state-specific linear time trends and (b) the parallel trends assumption holds. We then conduct two additional robustness tests: (1) we exploit the county-level data and employ a spatial regression discontinuity research design to compare neighboring counties subject to different interstate branching policies; and (2) we implement a correction to address potential biases associated with staggered difference-in-differences estimators

4.1. Baseline Results

We use a generalized *difference-in-difference* estimation strategy that exploits the staggered lowering of regulatory impediments to interstate bank branching to evaluate the impact of these regulatory reforms on mental depression. We use the following panel regression model:

$$Y_{i,s,t} = \alpha + \beta \cdot IBBEA\ Index_{s,t} + \theta \cdot Controls_{s,t} + \delta_i + \delta_{g,r,t} + N_t + T_s + \varepsilon_{i,s,t}, \qquad (1)$$

where $Y_{i,s,t}$ measures the mental health of individual *i* from state *s* at time *t*. The variable, $IBBEA\ Index_{s,t}$, is the interstate bank branching deregulation index for state *s* at time *t*, and it ranges from a value of zero to a value of four, where higher values indicate fewer restrictions on interstate branch banking. The coefficient, β, captures the impact of bank branch deregulation on mental health. The term, $Controls_{s,t}$, is a matrix of the *State-Year Controls* defined above, i.e., *Per-Capita GSP Growth*, *Log Population*, *Poverty Rate*, *Unemployment Rate*, *Labor Force Participation*, and *Log House Price Index*. We include individual fixed effects, δ_i, gender-race-year fixed effects ($\delta_{g,r,t}$), NLSY79 survey month and day fixed effects (N_t), and the state linear time trends (T_s). The *Survey Month and Day* fixed effects (N_t) are composed of two sets of time-varying dummy variables: the month and day the respondent completed the NLSY79 survey. We also consider a less restrictive specification, in which, instead of controlling for state linear trends and individual fixed effects, we control for state fixed effects and the time-invariant *Individual Controls* defined above, i.e., *Education*, *Age*, *Family Income*, *Mother's Education*, *Father's Education*, *D-Both Parents*, *AFQT Ability Test*, *Rosenberg Self-Esteem*, *Pearlin Sense of Control*, and *D-Risk Attitude* scores. Our results are robust under these alternative specifications. All regressions use the survey weights provided by NLSY79 to adjust for non-response, clustering, and stratification issues inherent in survey data. Standard errors are clustered at the state level.

Table 2 presents the results from estimating equation (1) using various combinations of controls and different subsamples of individuals. We report only the coefficient estimates on IBBEA Index for brevity, but Online Appendix Table OA1 provides the full regression results. All regressions control for *State-Year Controls*, *Gender-Race-Year* fixed effects (*FE*), and *Survey Month and Day FE*. The gender-race-year effects control for time-varying factors that differentially affect sub-populations. The survey month and day effects control for seasonal factors shaping mental well-being. Columns (1) - (3) also condition on *Individual Controls* and *State* fixed effects, where *State* controls for all time-invariant features of each state that could shape mental health. Columns (4) – (6) also control for *Individual FE* (and *State Linear* time trends, which condition out all time-invariant individual-level characteristics shaping mental health and trends in mental health that could differ by state. Thus, the analyses in columns (4) – (6) control for more individual and state characteristics than the regressions in columns (1) – (3). Following Chetty, Hendren, Kline, and Saez (2014), we create low- and high-income subsamples by (1) computing average total income (in 2010 dollars) over the three-year window immediately before our sample period (i.e., 1989-1991), and (2) dividing each state's sample of individuals into low- and high-income subsamples based on the state median value of average total income over the 1989-1991 period.

Bank branch deregulation is associated with a sharp drop in mental depression among low-income individuals, as shown in columns (2) and (5). Whether using the specification with fewer controls (column 2) or that with a more extensive array of fixed effects (column 5), deregulation is associated with a significant drop in the *Depression Index* among low-income individuals. The estimated coefficient on *IBBEA Index* is also economically important. For example, consider the estimate from column (5). The point estimate for the low-income group is -0.6 implies that one more degree of interstate bank branch openness, i.e., an increase of the *IBBEA Index* of one, is associated with a 14% reduction in the *Depression Index* relative to the sample mean of 4.3 among low-income individuals (i.e., 0.14=0.6/4.3).

The results also demonstrate that the significant negative relationship between bank deregulation and mental depression is driven entirely by the sample of low-income individuals. The IBBEA Index does not enter significantly in the high-income regressions (columns 3 and 6). Furthermore, the Wald statistic indicates that the data reject the hypothesis that the estimated coefficients on the IBBEA Index are equal in the high- and low-income subsamples at the five percent significance level.

The finding that branch deregulation reduces mental depression holds when implementing three sensitivity analyses, as reported in the Online Appendix Tables OA2. First, the results hold when (1) excluding *State-Year Controls* (column 1); (2) excluding survey weights (column 2); (3) including only individual and year fixed effects, i.e., including only "Basic" difference-in-differences fixed effects (column 3); (4) adding age and birth cohort fixed effects (column 4); and (5) using standardized versions of the *Depression Index* and *IBBEA Index*, with zero means and standard deviations of one (column 5). Second, the results hold when including people who move between states during the sample period (column 6). Third, the results hold when examining the dummy variable, *Depression Index* $\geq$ *16*, which indicates whether the individual has a *Depression Index* value equal to or greater than 16, a cut-off value used to detect major depression syndrome and disorder (Juarros-Basterretxea et al. 2021). Using this binary measure of major depression syndrome and disorder as the dependent variable also offers an additional way to gauge the magnitude of the estimated relationship between interstate bank branch deregulation and mental health. In particular, the estimates imply that reducing the IBBEA index by one reduces the incidence of major depression syndrome and disorder among low-income individuals by two percentage points. This is a large effect, as the average value of *Depression Index* $\geq$ *16* in the sample of low-income workers is 3.8%.[10] Although we cannot

[10] We use a linear probability model because the logit model is not econometrically consistent when using high-dimensional fixed effects with small number of sample periods (which is the case in our main model specification). However, when using several streamlined versions of the fixed effect controls (e.g., if we simply control for individual and year fixed effects), the logit

eliminate identification concerns, the extensive set of conditioning variables and the additional empirical methods that we now describe ameliorate such concerns.

4.2. Graphical Analyses

Figure 2 provides graphical evidence of the validity of our *difference-in-difference* methodology. It traces the evolution of the *Depression Index* for two groups of low-income individuals: those living in states that ultimately have high values of the IBBEA Index (i.e., values of 3 or above) after interstate bank branch deregulation (States Deregulating More) and those residing in the other states (*States Deregulating Less*). For each individual, the NLSY79 provides two observations of the *Depression Index* before the 1994 Riegle-Neal Act and one observation afterward. Figure 2 plots the average values of the *Depression Index* for low-income individuals for each of these three observations while splitting the sample between those living in *States Deregulating More* and *States Deregulating Less.* We also provide 95% confidence intervals around each of these average values.

Figure 2 illustrates two key findings. First, the parallel trends assumption holds: before deregulation, low-income individuals in states that ultimately deregulate more have almost the same trends in mental depression as those that eventually deregulate less. Second, we observe a sharp break (a sharp improvement) in those mental health trends following deregulation among low-income individuals living in states that deregulate more, but not among those living in states that deregulate less. This illustrates the strong connection between deregulation and improvements in mental health.

4.3. Robustness: Spatial Regression Discontinuity Analyses

We next employ a spatial regression discontinuity design to address additional identification concerns. Huang (2008) argues that geographically proximate counties across a shared state border are likely to have similar observable (e.g., those captured by the matrix of time-varying state traits, *State-Year Controls*) and unobservable—and hence omitted—traits. As a result, adjacent counties with different bank branch regulations can be good "matches" and

produces consistent results concerning statistical significance and estimated magnitudes.

therefore provide an additional empirical setting for assessing the impact of changes in credit conditions on mental health. Employing this strategy, we compare changes in mental health in adjacent counties across state borders with different branching regulations.

To implement the empirical design, we obtain 1990 state- and county-boundary data from the U.S. Census and create contiguous county pairs across state borders.[11] We then map this county-pair data into our NLSY79 sample (where we use restricted-access data on location), limit the sample to adjacent county pairs, and redo the main tests presented above. We control for county-pair, individual, gender-race-year, and survey month and day fixed effects and state-specific linear time trends. Standard errors are clustered at the county-pair level.

As shown in Panel A of Table 3, the results from spatial regression discontinuity analyses confirm the analyses above: interstate branch deregulation is associated with a sharp reduction in mental illness among low-income individuals but not high-income individuals. Among the low-income group distributed in contiguous counties along the state border, an increase in bank branch deregulation is associated with reduced mental depression.

4.4. Robustness: Difference-in-Differences Correction

Several prominent studies suggest that staggered difference-in-differences (DID) regressions can yield biased estimates, e.g., de Chaisemartin and D'Haultfoeuille (2020, 2023), Borusyak, Jaravel, and Spiess (2021), and Baker et al. (2022), and offer strategies for correcting such biases. However, these biases are unlikely to arise in our analyses because our regressions do not have the staggered structure that gives rise to those biases. Specifically, the bias occurs when DID regressions compare a later-treated group (the treated) to an earlier-treated group, which the regression considers the control group. In our baseline analysis, we only observe the *Depression Index* in three survey waves: 1992, 1994, and the year immediately after the person becomes 40 years old, which occurs between 1998 and 2005. Since interstate branching deregulation occurred after 1994 for all states except Alaska, almost all individuals in our sample experienced at most one treatment, so there is not a staggered DID structure.

[11] https://www.census.gov/geo/maps-data/data/tiger-cart-boundary.html

Nevertheless, for robustness, we note that all results hold when (1) eliminating the few individuals from Alaska with two treated observations and (2) conducting the Borusyak, Jaravel, and Spiess (2021) correction for staggered DID analyses when pre-trends appear parallel, as shown in Figure 2. The correction is based on a 0/1 treatment effect. However, our treatment, the IBBEA Index, takes on values between 0 and 4. Thus, we create and examine three alternative 0/1 treatment measures based on the IBBEA Index. Specifically, we define these three 0/1 treatments as equal to one if (1) the IBBEA Index>0 (Column 1); (2) the IBBEA Index>1 (Column 2); (3) the IBBEA Index>2 (Column 3); or (4) the IBBEA Index>3 (Column 4), respectively. As shown in Panel B of Table 3, the results are robust to implementing the DID correction.

5. Channels and Heterogeneous Effects

Having shown that interstate bank branch deregulation is associated with mental health improvements among low-income individuals, we now (1) explore three interrelated channels through which branch deregulation may enhance mental health and (2) examine how the relationship between bank deregulation and mental health differs by household and community characteristics.

5.1. Channels

5.1.1 Labor market outcomes

The labor market channel holds that branch deregulation eased firm credit constraints and improved workers' labor market outcomes, especially those of lower-income workers, positively impacting their mental health. The household credit channel holds that branch deregulation eased household credit constraints, especially those constraining lower-income families, allowing those households to purchase homes and more easily smooth shocks, boosting mental health. The non-credit banking services channel holds that interstate bank branch deregulation improved access to saving accounts and other financial services among low-income individuals with positive effects on mental health.

We begin by evaluating a previously unexamined component of the labor market channel. Rice and Strahan (2010) convincingly establish that branch deregulation eased firm credit conditions by reducing the interest rates charged by banks on loans. However, they do not examine labor market outcomes. Beck, Levine, and Levkov (2010) find that interstate bank deregulation disproportionately boosted the amount that low-income individuals work. However, they examine interstate bank deregulation, not branch deregulation. In this subsection, we evaluate the impact of interstate branch deregulation on labor market outcomes while differentiating between low- and high-income individuals.

We examine five labor market outcome measures. Three are measured at an annual frequency, *Log Work Weeks (Annual)*, (2) *Log Work Hours (Annual)*, and (3) *Log Wage Income (Annual)*, and two at a weekly frequency: *Is Working (Weekly)* and *Log Work Hours (Weekly)*) as we do not have weekly data on earnings. For the regressions at a weekly frequency, we use the specific week (rather than the year) of each interstate bank branch regulatory reform by state. For each of these labor market outcomes, we examine low- and high-income subsamples using the Equation (1) regression model and report the results in Table 4.

Interstate bank branch deregulation is associated with a statistically significant and economically large increase in labor market outcomes among low-income individuals. Specifically, among low-income individuals, deregulation boosted (1) log work weeks per annum, (2) log work hours per annum, (3) log work hours per week, (4) whether the person works during a week, and (5) log wage income. Consistent with the core findings on mental health presented above and the labor market outcome channel, we find no effect of branch deregulation on the employment outcomes of high-income workers. The impact on labor market outcomes is economically substantial. For example, when the *IBBEA Index* increases by one, there is a 5.2% increase in the number of weeks, on average, that low-income workers are employed during the year (column 1) and a 3.6% increase in wage earnings (column 9), consistent with the findings in Beck, Levine, and Levkov (2010).

5.1.2 Household credit

Interstate bank branch deregulation may also shape mental health by easing household credit constraints. To shed suggestive empirical light on this channel, we examine the three measures of individual borrowing collected by the NLSY79 during the sample period: *Log Mortgage Debt*, *Log Car Loans*, and *Log Other Debt > $500*.[12] We evaluate the relationship between deregulation and these debt measures using Equation (1).

We find that deregulation is associated with a statistically significant increase in mortgage debt among low-income individuals but not among high-income workers, as shown in Table 5. Thus, among low-income individuals, branch deregulation (a) increased mortgage debt (Table 5) and (b) reduced depression (Tables 2 and 3). Branch deregulation is not associated with significant changes in car loans or other debts in either low- or high-income subsamples. Although we cannot rule out the possibility that increases in mortgage debt boosted financial fragility and hurt mental health, we can conclude that, on average, interstate branch deregulation both increased mortgage debt and enhanced mental health among low-income individuals.

5.1.3 Savings account

The non-credit banking services channel holds that interstate bank branch deregulation improved mental health by expanding access to non-credit banking services among low-income individuals. We use data from the following NLSY79 survey question to gauge such access to banking services: "Do you [or your (husband/wife)] have any money assets like savings accounts?" We then test whether bank deregulation increased access to such accounts using regression Equation (1). Since (essentially) all high-income individuals have access to such accounts and therefore answer "yes," we run these regressions only for the sample of low-income individuals. Consistent with the findings by Célerier and Matray (2019), we find that interstate branch deregulation boosted access to saving accounts among low-income households who did not have such accounts before the deregulation, as shown in Table 6.

5.1.4 Two-Stage analyses

Next, we use a two-stage approach to provide additional information on the channels linking bank deregulation and mental health, though we stress the limitations of these analyses.

[12] The NLSY79 does not provide data on the interest rates charged on these loans or personal bankruptcies during our sample.

For each channel, the first stage uses the interstate bank branch deregulation (IBBEA) index as an “instrument”. Specifically, we collect the predicted values from the first columns of Tables 4, 5, and 6 and use them in the second stage, reporting bootstrapped standard errors clustered at the state level. In this way, we assess the connection between mental health and three potential channels through which interstate branch deregulation may influence mental health. We provide the results for the sample of low-income individuals, as our analyses demonstrate that deregulation has a significant, robust relationship with mental health only among these individuals.

Table 7 shows that the second-stage results are consistent with the view that interstate branch deregulation improves mental health by boosting the labor market outcomes of low-income individuals, as predicted *Log Work Weeks* enters negatively and significantly. However, neither predicted *Log Mortgage Debt* nor predicted *Savings Account* is significantly correlated with mental health.

While illustrative, these analyses must be viewed cautiously. Instrumental variable analyses assume that the instrument only affects the dependent variable through its impact on the endogenous variable. In our setting, we are assessing three potential channels, i.e., potential endogenous variables, through which our instrument could influence mental health. Although the second-stage results highlight the labor market outcome channel, we interpret these findings cautiously because we cannot rule out that bank deregulation affects mental health through mechanisms other than the labor market outcome channel, potentially violating the exclusion restriction.

5.2. Heterogeneous Effects of Interstate Branch Deregulation on Mental Health

The impact of interstate branch deregulation on mental health may depend on pre-existing household traits and features of the community in which individuals live. Consider first the role of household leverage in shaping how individuals respond to deregulation. While deregulation could improve welfare by enhancing access to credit, research also suggests that deregulation could harm individuals with high leverage if deregulation triggers overborrowing (e.g., Karlan and Zinman 2020, and Andersen et al. 2022). Furthermore, if households already have high leverage, deregulation is unlikely to boost mental health by easing borrowing constraints. From this perspective, the impact of deregulation on depression will be less negative, and perhaps even positive, among those with sufficiently high pre-treatment debt levels.

To assess the potential heterogeneous relationship between interstate branch deregulation and mental health based on leverage, we re-do the analyses while including an interaction term based on households' pre-treatment debt-to-income (DTI) ratios, where the pre-treatment DTI is computed as the average DTI level from 1989 through1991. We categorize a household as highly levered if its DTI is above the 75th percentile of the sample (*DTI>75%*), where DTI at the 75th percentile is about equal to one. We then include the interaction term, *IBBEA Index * DTI>75%*, into the regression analyses based on Equation (1).

The findings reported in Table 8 are consistent with the hypothesis that the relationship between interstate bank branch deregulation and mental health varies with pre-treatment household leverage. First, the negative relationship between deregulation and mental health dampens among households with higher debt levels. In fact, the estimated effect of interstate branch deregulation on depression becomes positive among these high-leverage households, as reflected by the sum of the coefficients on the linear (*IBBEA Index*) and interactive term (*IBBEA Index * DTI>75%*) in Column (1). The findings are consistent with the view that easing access to credit for households with already high leverage ratios does not improve mental health and may harm mental health at high enough debt levels. Second, consistent with our core results, we find a robust negative relationship between bank deregulation and mental depression among low-income households that were not highly levered before deregulation.

Next, consider the potential impact of two community characteristics—income inequality and political ideology—in shaping how deregulation affects mental health. On income inequality, suppose interstate branch deregulation disproportionately boosts the employment opportunities of low-income individuals. Suppose further that having a low income exerts an especially adverse impact on mental health among those living in high-income inequality communities, as suggested by findings in Wilkinson (2002). Under those conditions, branch deregulation would reduce depression more among low-income individuals in high-income inequality states. To test this hypothesis, we redo the analyses while including the interaction between the state's pre-treatment Gini coefficient of income inequality (*Gini*) and the *IBBEA Index*. As shown in Column (2) of Table 8, the negative relationship between bank deregulation and depression among low-income individuals is stronger in states with greater pre-treatment income inequality.

Finally, consider political ideology. Research suggests that those on the political left value group-based income equality more positively than those on the right (e.g., Waldfogel et al. 2021; Goldman et al. 2021; Lin et al. 2023). Under these conditions, interstate bank branch deregulation, which disproportionately boosts the employment opportunities of low-income individuals, would tend to improve mental more among those who care more about relative income, i.e., low-income, democratic households. Although we do not know the political affiliations of each household, we do have information on the pre-treatment percentage of Democratic voters as captured by the state-level percentage of votes for Dukakis (*%Democrat*), who was the 1988 Democratic presidential candidate. We then redo the baseline analyses while including the interaction between the *IBBEA Index* and *%Democrat*. Consistent with the view that the impact of deregulation on mental health depends on the political ideology of the community, we find a stronger relationship between interstate bank branch deregulation and mental health among lower-income households living in more Democratic states, as shown in Column (3) of Table 8.

6. A Reverse Quasi-Natural Experiment: Bank Mergers

Finally, we evaluate the relationship between credit conditions and mental health using a different dataset that (1) covers a different sample period, (2) employs a different shock to local credit conditions, and (3) uses a different measure of mental health. Rather than assessing the impact of interstate bank deregulation that improved local credit conditions, we examine the effects of shocks that (a) occurred over a later period and (b) tightened credit conditions. We test whether these adverse shocks to locale credit conditions had deleterious effects on distinct measures of the mental health of low-income individuals living in those communities.

To test this prediction, we begin with the work of Nguyen (2019), who shows that consolidating local banking markets through the closing of branches tightened credit constraints on small, local businesses. This finding is consistent with extensive research demonstrating that small firms depend more heavily than large banks on geographically close banks for credit. Geographic proximity helps those banks ameliorate the informational asymmetries associated with lending to small firms. As an instrument for consolidating and closing bank branches within a local market, we follow Nguyen (2019) and use the mergers of large, multi-state banks with branches in that local market. Our identifying assumption is that bank mergers occurring at the regional, multi-state level are exogenous to the county-level mental health conditions in which those banks have branches.[13] We then assess the impact of merger-induced bank consolidations on the mental health of individuals within treated locales.

We can no longer use the NLSY79 to measure mental health because it has only two observations for each respondent during the 2001-2017 period. Thus, we turn to the U.S. Panel Study of Income Dynamics (PSID), another representative panel dataset. We use the restricted PSID data, which reports the county-level geographic code for each respondent. The PSID does not offer as precise measures of depression as those provided by the NLSY79. However, the PSID collects valuable information on respondents' mental distress in 2001, 2003, and 2007-2017 biennially. Each household is surveyed about mental health once per wave. Using the abovementioned methodology, we partition the total sample (household heads at the prime age between 25-60) into high- and low-income subsamples.

To measure mental health using the PSID, we use the *K6 Distress Scale*. It ranges from 0 (least distressed) to 24 (most distressed). Respondents were asked to choose from 0 (None of the time) to 4 (All of the time) about the degree to which they agreed with the following six statements over the past month: (1) I felt so sad that nothing could cheer me up (*Feel Sad*), (2) I felt nervous (*Feel Nervous*), (3) I felt restless or fidgety *(Feel Fidgety*), (4) I felt hopeless (*Feel Hopeless*), (5) I felt that everything was an effort (*Everything an Effort*), and (6) I felt worthless (*Feel Worthless*). From this information, we create two dummy variables to evaluate the severity of mental distress. *D-K6 Scale Above 5* measures the incidence of moderate distress, and it is a dummy variable that equals one if *K6 Distress Scale* is above 5 and zero otherwise. *D-K6 Top*

[13] Nguyen (2019) uses census tract data on branches, small-business lending, and mortgage lending. We do not have information about individuals' mental health at the census tract level, so we analyze county-level data.

Quartile measures the incidence of severe distress and equals one if *K6 Distress Scale* falls in the top quartile and zero otherwise. We also construct the variable *% of Answers "0"* to denote the intensity of mental distress, and it is the percentage of choosing 0 as the ratings of the six statements. PSID also asks about the real effect of each individual's mental distress: "During the past 30 days, how many days were you totally unable to work or carry out your normal activities because of these feelings." Using these data, we set *Days Unable to Work* equal to the natural logarithm of suffering days.

For the merger-induced local branch consolidation measure, we obtain the U.S. banks' merger and acquisition deals from the National Information Center (NIC) and assets information from the Federal Deposits Insurance Corporation (FDIC) between 2001 and 2017. To ensure that the bank's M&A decision is plausibly exogenous to local economic conditions, we only keep those deals in which the acquirer and target banks (1) each held at least $1 billion in assets before the merger, (2) were classified as "nonfailing," and (3) were owned by different BHCs. We also compile county-level bank branch data from FDIC Summary of Deposits (SOD) and define *Treat* as one if the county has branches from both the acquirer and target banks in the year before the merger and zero otherwise. Online Appendix Table OA4 summarizes the data used in the merger-induced analyses.

We first test whether the Nguyen-type merger shocks are associated with a tightening of local credit conditions during our sample period using the following regression specification.

$$Y_{s,c,t} = \alpha + \beta \cdot Treat_c \cdot Post_t + \delta_c + \delta_{s,t} + \varepsilon_{s,c,t}, \qquad (2)$$

where $Y_{s,c,t}$ is a measure of small-business loans in county *c* within state *s* at time *t*. *Post* is a dummy variable that equals one following the merger year, and zero otherwise. β captures the relationship between small-business lending in a county and the county's exposure to merger shocks. Since this variation is at the county-year level, we can control for state-year fixed effects, $\delta_{s,t}$. Standard errors are clustered at the county level. To measure small-business loans, we use data from the Community Reinvestment Act (CRA) on loans originated to businesses with gross annual revenues below $1 million by CRA-eligible banks (i.e., banks with at least $1 billion in assets).[14] We consider both the number and volume of small business loans. *Small Business Loans-Number* denotes the total number of small business loans (in thousands). *Small Business Loans-Amount* denotes the total amount of small business loans (in millions). We also use more refined measures of the number and dollar amount of small-business lending. On the number of loans, *Loan Number [<$100,000]* denotes the number (in thousands) of small business loans originated with loan origination values less than $100,000; *Loan Number [$100,000, $250,000]* denotes the number (in thousands) of small business loans originated with loan origination values between $100,000 and $250,000; and *Loan Number [$250,000, $1,000,000]* is defined similarly. On the dollar value of loans, *Loan Amount [<$100,000]* denotes the total dollar value (in millions) of small business loans originated with loan origination values less than $100,000, and *Loan Amount [$100,000, $250,000] and Loan Amount [$250,000, $1,000,000]* are defined similarly.

As shown in Table 9, merger shocks are associated with a tightening of local credit conditions. The number and amount of small business loans fall appreciably in counties with branches exposed to bank mergers. For example, small business loans are 25% (0.25=0.469/1.854) smaller in counties with merger shocks than among otherwise similar non-merger-shock counties. Furthermore, these results hold for different loan size categories.

We next test whether the merger shocks and the tightening of local credit conditions led to a decline in mental health among low-income individuals. We estimate a modified version of equation (2), where the dependent variable is now $Y_{i,s,c,t}$, which equals the mental health of low-income individual *i* from state *s* county *c* at time *t*. We include individual and gender-race-year fixed effects. All regressions use the longitudinal survey weights provided by PSID, and standard errors are clustered at the county level.

As shown in Table 10, the results confirm the earlier findings: local credit conditions materially influence the mental health of low-income individuals. Compared to the individuals in the control counties, those in the exposed counties—counties with a merger-induced shock to local branches—experience more mental distress (column (1)). Their K6 distress scale increases by about 26% (0.877/3.415), and they are more likely to become moderately or severely

[14] Since CRA data started in 1997, we cannot use it for the bank branch deregulation analyses.

distressed (columns (2) and (3)) and choose less "None of the time" as their answers to each statement (column (4)). Consistent with the analyses using interstate branch deregulation, we find that merger shocks only reduced the mental health of lower-income individuals, as shown in the Online Appendix Table OA5.

7. Conclusion

In light of conflicting predictions about the impact of credit conditions on mental health, we assess how bank regulatory reforms that improved credit conditions—and large regional bank mergers that triggered local branch closures and tighter local credit conditions—influenced mental health. We discover that bank regulatory reforms that improved credit conditions boosted the mental health of low-income individuals but not higher-income workers. This finding is consistent with past work showing that regulatory reforms that enhanced credit conditions disproportionately increased the demand for lower-income workers (e.g., Beck, Levine, and Levkov 2010). Using a different shock to credit conditions and a different measure of mental health, we find that bank mergers that tightened local credit conditions harmed mental health among low-income individuals affected by the mergers. These findings suggest that credit conditions have substantial implications on mental health.

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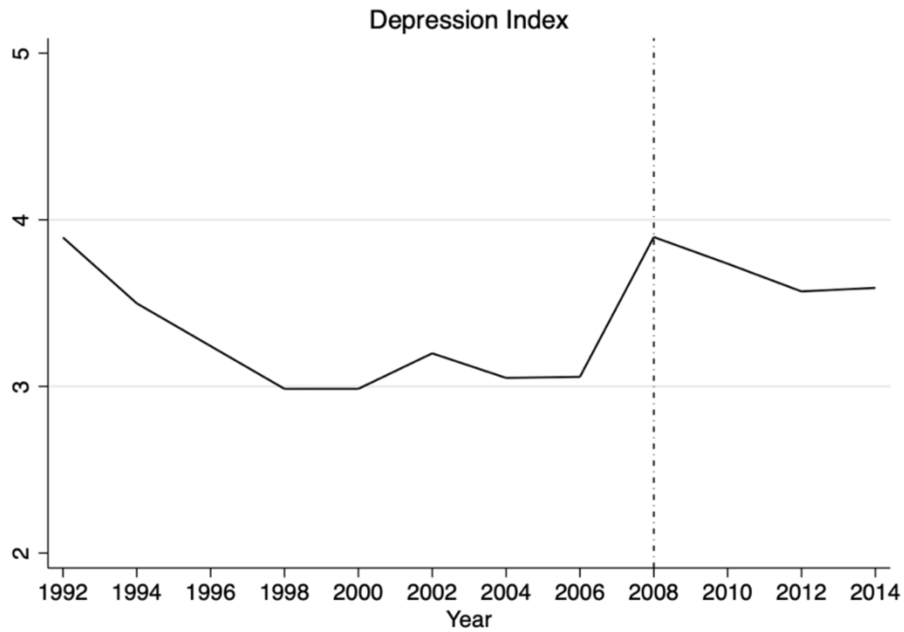

Figure 1. Time Series of the Depression Index

This figure plots the average value of the *Depression Index* across the NLSY79 survey waves from 1992 to 2014.

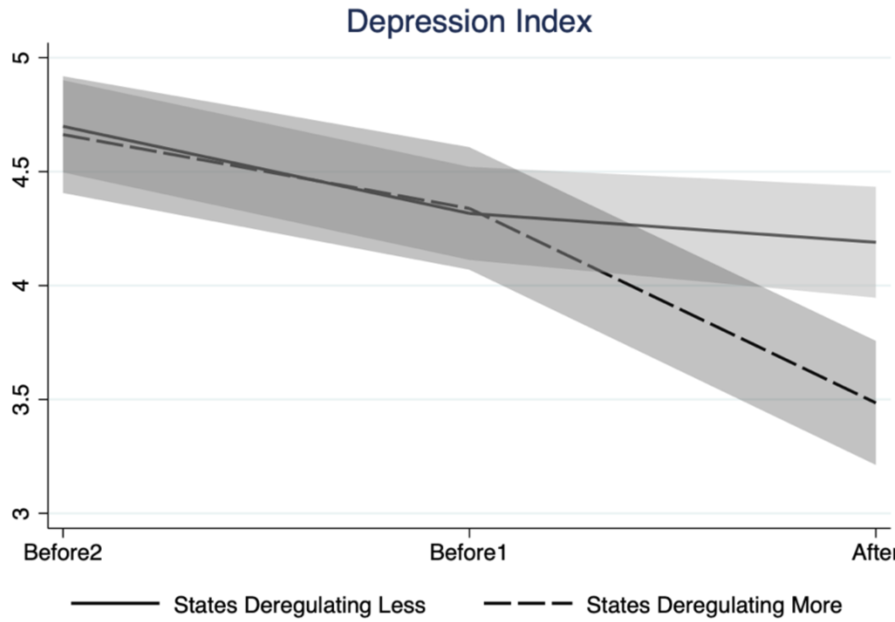

Figure 2. Evolution of the Depression Index by Bank Deregulation

This figure traces the evolution of the Depression Index for two groups of low-income individuals: those living in states that ultimately have high values of the IBBEA Index (i.e., values of 3 or above) after interstate bank branch deregulation (States Deregulating More) and those residing in the other states (States Deregulating Less). For each individual, the NLSY79 provides two observations of the Depression Index before the 1994 Riegle-Neal Act ("Before2" and "Before1" in the figure) and one observation afterward, which is denoted by "After" in the figure. Figure 2 plots the average values of the Depression Index for low-income individuals for each of these three observations (Before2, Before1, and After) while splitting the sample between those living in States Deregulating More and States Deregulating Less. The shaded areas are the 95% confidence intervals around these average values.

Table 1. Summary Statistics

This table provides summary statistics on critical variables. Appendix 1 provides variable definitions.

Variable	N	Mean	S.D.	Min	Median	Max
State-Level Bank Branching Deregulation						
IBBEA Index	714	1.318	1.496	0	1	4
Individual-Level CES-Depression Scale						
Depression Index	14,291	3.480	3.939	0	2	21
Low-Income Group	6,576	4.347	4.503	0	3	21
High-Income Group	7,715	2.858	3.344	0	2	21
Depression Index>=16	14,291	0.021	0.144	0	0	1
Low-Income Group	6,576	0.038	0.191	0	0	1
High-Income Group	7,715	0.009	0.094	0	0	1
Individual-Level Labor Market Outcomes						
Log Work Weeks (Annual)	26,373	3.459	1.230	0	3.970	3.970
Log Work Hours (Annual)	26,095	6.729	2.376	0	7.641	8.120
Is Working Dummy (Weekly)	1,140,638	0.853	0.354	0	1	1
Log Work Hours (Weekly)	1,134,906	3.171	1.364	0	3.714	4.304
Log Wage Income (Annual)	25,369	3.033	1.418	0	3.532	4.593
Individual-Level Access to Credit & Banking Services						
Log Mortgage Debt	20,000	2.399	2.208	0	3.274	5.232
Log Car Loans	17,072	1.195	1.302	0	0.483	3.468
Log Other Debt >$500	20,172	0.693	1.035	0	0.000	3.310
Has Savings Account	20,517	0.779	0.415	0	1	1

Table 2. Bank Branch Deregulation and Depression

This table reports OLS regression results of the respondent's *Depression Index* on interstate bank branch deregulation (*IBBEA Index*) and extensive controls for various subsamples of respondents. Each respondent has three observations for the *Depression Index*: 1992, 994, and when turning 40. All regressions control for State-Year Controls (*Per-capita GSP Growth, Log Population, Poverty Rate, Unemployment Rate, Labor Force Participation Rate,* and *Log House Price Index*) and an array of fixed effects (*Gender-Race-Year and Survey Month and Day*). In addition, regressions 1-3 also control for numerous time-invariant individual traits defined in the text (Individual Controls) and State fixed effects, and regressions 4-6 also control for individual fixed effects and state linear time trends. Columns (1) and (4) report results for the full sample, columns (2) and (5) for low-income respondents, and (3), and (6) for high-income individuals, respectively. Appendix 1 provides detailed variable definitions. All regressions use sample weights provided by NLSY79. Heteroskedasticity-robust standard errors clustered at the state level are in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%. The last row reports the p-values for the Wald test comparing the effect of the *IBBEA Index* on the *Depression Index* in the low and high-income groups.

	Depression Index					
	Full Sample	Low Income	High Income	Full Sample	Low Income	High Income
	(1)	(2)	(3)	(4)	(5)	(6)
IBBEA Index	-0.110	-0.269**	-0.019	-0.285***	-0.602***	-0.112
	(0.079)	(0.114)	(0.075)	(0.101)	(0.157)	(0.100)
Individual Controls	Yes	Yes	Yes			
Individual FE				Yes	Yes	Yes
State FE	Yes	Yes	Yes			
State Linear Trends				Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes
Gender-Race-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Survey Month and Day FE	Yes	Yes	Yes	Yes	Yes	Yes
Observation	14,225	6,545	7,680	14,202	6,478	7,608
R-Squared	0.141	0.146	0.112	0.605	0.620	0.573
Wald Test (P-Value)		0.012			0.008	

Table 3. Robustness: Spatial Regression Discontinuity and DID Correction

This table conducts robustness checks. Panel A reports regression results of the respondent's *Depression Index* on interstate bank branch deregulation (*IBBEA Index*) and extensive controls using a spatial regression discontinuity design. The sample only includes observations in contiguous county pairs across state borders. The regressions control for State-Year Controls (*Per-capita GSP Growth, Log Population, Poverty Rate, Unemployment Rate, Labor Force Participation Rate,* and *Log House Price Index*), state linear time trends, and an array of fixed effects (*County-Pair, Individual, Gender-Race-Year,* and *Survey Month and Day*). Column (1) reports results for the full sample, and columns (2) and (3) for low- and high-income individuals, respectively. The last row reports the p-values for the Wald test comparing the effect of the *IBBEA Index* on the *Depression Index* in the low and high-income groups. Panel B conducts robustness checks of the difference-in-differences (DID) regressions for the sample of low-income respondents using the correction developed by Borusyak, Jaravel, and Spiess (2021). Since the correction is based on a 0/1 treatment effect, we define the treatment as: (1) the IBBEA Index>0 (Column 1); (2) the IBBEA Index>1 (Column 2); (3) the IBBEA Index>2 (Column 3) and (4) the IBBEA Index>3 (Column 4). The regressions control for the indicated controls. Appendix 1 provides detailed variable definitions. All regressions use sample weights provided by NLSY79. Heteroskedasticity-robust standard errors clustered at the county-pair (Panel A) or state (Panel B) level are in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%.

Panel A: Regression Discontinuity	Depression Index		
	Full Sample	Low Income	High Income
	(1)	(2)	(3)
IBBEA Index	-0.322*	-0.732**	-0.012
	(0.166)	(0.287)	(0.196)
County-Pair FE	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
State Linear Trends	Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes
Gender-Race-Year FE	Yes	Yes	Yes
Survey Month and Day FE	Yes	Yes	Yes
Observation	10,383	4,942	5,362
R-Squared	0.680	0.737	0.644
Wald Test (P-Value)		0.034	

Panel B: DID Correction	Depression Index			
	(IBBEA>0)	(IBBEA>1)	(IBBEA>2)	(IBBEA>3)
	(1)	(2)	(3)	(4)
Treatment	-0.951***	-0.833***	-0.778**	-0.783**
	(0.341)	(0.294)	(0.339)	(0.347)
Individual FE	Yes	Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes	Yes
Gender-Race-Year FE	Yes	Yes	Yes	Yes
Survey Month and Day FE	Yes	Yes	Yes	Yes
Observation	6,506	6,527	6,532	6,538

Table 4. Labor Market Outcomes

This table presents OLS regression results of labor market outcomes on interstate bank branch deregulation (*IBBEA Index*) and extensive controls for samples of low- and high-income respondents. Of the five labor market outcome measures, three are measured at an annual frequency: (1) the natural logarithm of one plus the total number of weeks the respondent is employed (*Log Work Weeks (Annual)*), (2) the natural logarithm of one plus the total number of hours the respondent is employed (*Log Work Hours (Annual)*), and (3) the natural logarithm of one plus the respondent's annual real wage (*Log Wage Income (Annual)*); and two are measured at a weekly frequency: (1) a dummy variable that equals one if the respondent works during the week (*Is Working (Weekly)*) and (2) the natural logarithm of one plus the total number of hours the respondent works during the week (*Log Work Hours (Weekly)*). For the regressions at a weekly frequency, we use the specific week (rather than the year) of each state's interstate bank branch regulatory reform. The regressions control for State-Year Controls (*Per-capita GSP Growth, Log Population, Poverty Rate, Unemployment Rate, Labor Force Participation Rate,* and *Log House Price Index*), state linear time trends, and an array of fixed effects (*Individual, Gender-Race-Year (or Week),* and *Survey Month and Day*). Appendix 1 provides detailed variable definitions. All regressions use sample weights provided by NLSY79. Heteroskedasticity-robust standard errors clustered at the state level are in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%. The last row reports the p-values for the Wald test comparing the effect of the *IBBEA Index* on the *Depression Index* in the low and high-income groups.

	Log Work Weeks (Annual)		Log Work Hours (Annual)		Is Working (Weekly)		Log Work Hours (Weekly)		Log Wage Income (Annual)	
	Low Income	High Income	Low Income	High Income	Low Income	High Income	Low Income	High Income	Low Income	High Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
IBBEA Index	0.052***	0.013	0.098***	0.015	0.012**	-0.002	0.045**	-0.006	0.036**	0.011
	(0.014)	(0.010)	(0.025)	(0.023)	(0.005)	(0.003)	(0.018)	(0.011)	(0.017)	(0.022)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Gender-Race-Year FE	Yes	Yes	Yes	Yes	No	No	No	No	Yes	Yes
Gender-Race-Week FE	No	No	No	No	Yes	Yes	Yes	Yes	No	No
Survey Month and Day FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observation	12,203	14,009	12,081	13,855	502,870	637,768	500,580	634,326	11,657	13,652
R-Squared	0.622	0.467	0.631	0.483	0.519	0.376	0.544	0.404	0.661	0.498
Wald Test (P-Value)	0.031		0.015		0.007		0.007		0.141	

Table 5. Household Credit Outcomes

This table presents OLS regression results of household credit outcomes on interstate bank branch deregulation (*IBBEA Index*) and extensive controls for samples of low- and high-income respondents. The dependent variable is either (1) the natural logarithm of one plus mortgage debt (*Log Mortgage Debt*), (2) the natural logarithm of one plus car loans (*Log Car Loans*), and (3) the natural logarithm of all other debts greater than $500 (*Log Other Debt > $500*). The regressions control for State-Year Controls (*Per-capita GSP Growth, Log Population, Poverty Rate, Unemployment Rate, Labor Force Participation Rate,* and *Log House Price Index*), state linear time trends, and an array of fixed effects (*Individual, Gender-Race-Year,* and *Survey Month and Day*). Appendix 1 provides detailed variable definitions. All regressions use sample weights provided by NLSY79. Heteroskedasticity-robust standard errors clustered at the state level are in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%. The last row reports the p-values for the Wald test comparing the effect of the *IBBEA Index* on the *Depression Index* in the low and high-income groups.

	Log Mortgage Debt		Log Car Loans		Log Other Debt > $500	
	Low Income	High Income	Low Income	High Income	Low Income	High Income
	(1)	(2)	(3)	(4)	(5)	(6)
IBBEA Index	0.038**	-0.003	0.014	-0.004	-0.020	-0.016
	(0.019)	(0.016)	(0.025)	(0.017)	(0.017)	(0.023)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
State Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes
Gender-Race-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Survey Month and Day FE	Yes	Yes	Yes	Yes	Yes	Yes
Observation	9,356	10,582	6,890	10,018	9,432	10,685
R-Squared	0.76	0.674	0.504	0.457	0.455	0.459
Wald Test (P-Value)	0.138		0.598		0.896	

Table 6. Savings Account

This table presents OLS regression results of access to saving accounts on interstate bank branch deregulation (*IBBEA Index*) and extensive controls for the sample of low-income respondents. The dependent variable, *Has Savings Account*, is a dummy variable based on respondents' answers to the following survey question: "Do you [or your (husband/wife)] have any money assets like savings accounts?" Column (1) focuses on the sample of low-income respondents who did not have access to savings account before 1992, and Column (2) focuses on those who had. The regressions control for State-Year Controls (*Per-capita GSP Growth, Log Population, Poverty Rate, Unemployment Rate, Labor Force Participation Rate,* and *Log House Price Index*), state linear time trends, and an array of fixed effects (*Individual, Gender-Race-Year,* and *Survey Month and Day*). Appendix 1 provides detailed variable definitions. All regressions use sample weights provided by NLSY79. Heteroskedasticity-robust standard errors clustered at the state level are in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%.

	Has Savings Account	
	No Savings Account Ex Ante	Has Savings Account Ex Ante
	(1)	(2)
IBBEA Index	0.032**	-0.003
	(0.013)	(0.007)
Individual FE	Yes	Yes
State Linear Trends	Yes	Yes
State-Year Controls	Yes	Yes
Gender-Race-Year FE	Yes	Yes
Survey Month and Day FE	Yes	Yes
Observation	2,016	7,522
R-Squared	0.475	0.551

Table 7. Instrumental Variable Tests of Potential Channels

This table presents the second-stage regression results assessing the channels connection between interstate bank branch deregulation and the *Depression Index* for the sample of low-income respondents. The instrumented variables are *Log Work Weeks*, *Log Mortgage Debt* and *Has Savings Account*, respectively. The instrumental variable is the *IBBEA Index*. The first-stage results are reported in the first column of Tables 4, 5, and 6. Column (3) includes only the subsample of respondents that did not have a savings account before 1992. The regressions control for State-Year Controls (*Per-capita GSP Growth*, *Log Population*, *Poverty Rate, Unemployment Rate, Labor Force Participation Rate,* and *Log House Price Index*), state linear time trends, and an array of fixed effects (*Individual, Gender-Race-Year,* and *Survey Month and Day*). Appendix 1 provides detailed variable definitions. All regressions use sample weights provided by NLSY79. Bootstrapped standard errors clustered at the state level are in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%.

	Depression Index		
	Labor market Channel	HH Credit Channel	Banking Service Channel
	(1)	(2)	(3)
Log $\widehat{\text{Work}}$ Weeks	-10.526**		
	(5.139)		
Log $\widehat{\text{Mortgage}}$ Debt		-14.941	
		(21.631)	
Has $\widehat{\text{Savings}}$ Account			1.484
			(185.600)
Individual FE	Yes	Yes	Yes
State Linear Trends	Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes
Gender-Race-Year FE	Yes	Yes	Yes
Survey Month and Day FE	Yes	Yes	Yes
Observation	4,515	3,789	797
R-Squared	0.628	0.677	0.805

Table 8. Heterogeneity by Household Leverage and Community Characteristics

This table reports regression results of the respondent's *Depression Index* on interstate bank branch deregulation (*IBBEA Index*) and extensive controls while including interaction terms to assess how ex-ante household leverage, local income inequality, and political affiliation shape the relationship between the *IBBEA Index* and mental health for the sample of low-income respondents. In column (1), the interaction term is *IBBEA Index * DTI>75%*, where *DTI>75%* is a dummy variable that equals one if the respondent's pre-treatment (1989-1991) debt-to-income (DTI) ratio is above the 75th percentile of the sample (*DTI>75%*), where DTI at the 75th percentile is about equal to one. In column (2), the interaction term is *IBBEA Index * Gini*, where *Gini* is the state-level average Gini index between 1989 and 1991, de-meaned and multiplied by 100. In column (3), the interaction term is *IBBEA Index * %Democrat*, where *%Democrat* is the state-level share of votes for the Democratic presidential candidate in the 1988 election, de-meaned and multiplied by 100. The regressions control for State-Year Controls (*Per-capita GSP Growth*, *Log Population*, *Poverty Rate*, *Unemployment Rate*, *Labor Force Participation Rate*, and *Log House Price Index*), state linear time trends, and an array of fixed effects (*Individual*, *Gender-Race-Year*, and *Survey Month and Day*). In the first column where the grouping indicator is a household-level indicator, we include the full set of interactions between DTI>75% and the state-year controls, state linear trends, and relevant fixed effects. Appendix 1 provides detailed variable definitions. All regressions use sample weights provided by NLSY79. Heteroskedasticity-robust standard errors clustered at the state level are in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%.

	Depression Index		
	By Ex-ante HH Leverage	By Ex-ante Income inequality	By Ex-ante Political Affiliation
	(1)	(2)	(3)
IBBEA Index	-1.255***	-0.698***	-0.625***
	(0.227)	(0.166)	(0.169)
IBBEA Index * DTI>75%	1.775***		
	(0.360)		
IBBEA Index * Gini		-0.208**	
		(0.092)	
IBBEA Index * %Democrat			-0.060**
			(0.024)
Individual FE	Yes	Yes	Yes
State Linear Trends	Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes
Gender-Race-Year FE	Yes	Yes	Yes
Survey Month and Day FE	Yes	Yes	Yes
Observation	3,773	6,478	6,478
R-Squared	0.663	0.620	0.621

Table 9. Bank Branch Closures and Credit Supply

This table reports OLS regression results of credit supply on bank branch closures from 2001 to 2017, using credit data from the Community Reinvestment Act. *Small Business Loans-Number* denotes the total number of small business loans (in thousands). *Small Business Loans-Amount* denotes the total loan amount of small business loans (in millions *Loan Number [<$100,000]* denotes the number (in thousands) of small business loans originated with loan origination values less than $100,000; *Loan Number [$100,000, $250,000]* denotes the number (in thousands) of small business loans originated with loan origination values between $100,000 and $250,000; and *Loan Number [$250,000, $1,000,000]* is defined similarly. *Loan Amount [<$100,000]* denotes the total dollar value (in millions) of small business loans originated with loan origination values less than $100,000; and *Loan Amount [$100,000, $250,000] and Loan Amount [$250,000, $1,000,000]* are defined similarly. *Treat* is a dummy variable that equals one if the county has branches from both the acquirer and target banks in the year before the merger and zero otherwise. *Post* is a dummy variable that equals one after the large banks merge and zero otherwise. We include county and state-year fixed effects in all columns. Heteroskedasticity-robust standard errors clustered at the county level are in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%.

	Small Business Loans-Number	Small Business Loans-Amount	Loan Number [<$100,000]	Loan Amount [<$100,000]	Loan Number [$100,000, $250,000]	Loan Amount [$100,000, $250,000]	Loan Number [$250,000, $1,000,000]	Loan Amount [$250,000, $1,000,000]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat × Post	-0.469***	-18.412***	-0.418***	-4.313***	-0.031***	-5.221***	-0.019***	-8.705***
	(0.065)	(2.467)	(0.061)	(0.727)	(0.003)	(0.542)	(0.003)	(1.337)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observation	54299	54299	54299	54299	54299	54299	54299	54299
R-Squared	0.949	0.974	0.946	0.959	0.962	0.963	0.973	0.974

Table 10. Bank Branch Closures and the K6-Distress Scale from the PSID

This table reports OLS regression results of the respondent's K6-Distress Scale on bank branch closures among low-income individuals from 2001 to 2017. *K6 Distress Scale* denotes the K6 non-specific psychological distress scale, ranging from 0 (least distressed) to 24 (most distressed). *K6 Distress Scale* comprises six items concerning the level of distress in the past 30 days; the answers range from 0 (None of the time) to 4 (All of the time). *D-K6 Scale Above 5* measures the incidence of moderate distress; it equals one if *K6 Distress Scale* is above 5, and zero otherwise. *D-K6 Top Quartile* measures the incidence of severe distress and equals one if *K6 Distress Scale* falls in the top quartile, and zero otherwise. *% of Answers "0"* denotes the percentage of the six questions the respondent answers "0," least distressed. *Treat* is a dummy variable that equals one if the county has branches from both the acquirer and target banks in the year before the merger and zero otherwise. *Post* is a dummy variable that equals one after the large banks merge and zero otherwise. We include individual, gender-race-year, and state-year fixed effects in all columns. All regressions use sample weights provided by PSID. Heteroskedasticity-robust standard errors clustered at the county level are in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%.

	K6 Distress Scale	D-K6 Scale Above 5	D-K6 Top Quartile	% of Answers "0"
	(1)	(2)	(3)	(4)
Treat × Post	0.877**	0.101**	0.074**	-0.072**
	(0.332)	(0.047)	(0.036)	(0.031)
Individual FE	Yes	Yes	Yes	Yes
Gender-Race-Year FE	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes
Observation	8182	8182	8182	8182
R-Squared	0.555	0.451	0.435	0.534

Appendix 1: Variable Definition

Variable	Definition	Source
State-Level Bank Branching Deregulation		
IBBEA Index	We follow RS (2010) and contrast the index ranging from 0 (most restrictive) to 4 (most deregulated).	Rice and Strahan (2010)
Merger-induced Local Branch Consolidation Measure		
Treat	We follow Nguyen (2019) and construct the dummy variable that equals to one if the county has branches from both the acquirer and target bank in the year prior to the merger, and zero otherwise.	NIC, SOD
Post	A dummy variable that equals one following the merger year, and zero otherwise.	NIC, SOD
CES-Depression Scale		
Depression Index	A total score of seven items measuring symptoms of depression, ranging from 0 (least depressed) to 21 (most depressed).	NLSY79
D-Depression Index Above 16	A dummy variable that equals one if Depression Index is above the cutoff 16, and zero otherwise.	NLSY79
K6 Distress Scale		
K6 Distress Scale	K6 non-specific psychological distress scale, ranging from 0 (lease distressed) to 24 (most distressed).	PSID
D-K6 Scale Above 5	A dummy variable that equals one if K6 Distress Scale is above 5, and zero otherwise.	PSID
D-K6 Top Quartile	A dummy variable that equals one if *K6 Distress Scale* falls in the top quartile, and zero otherwise.	PSID
% of Answers "0"	The percentage of answers choosing 0 as the ratings out of the six statements.	PSID
Labor Market Outcomes		
Log Work Weeks (Annual)	Natural logarithm of one plus total number of weeks employed in a given year.	NLSY79
Log Work Hours (Annual)	Natural logarithm of one plus total number of work hours in a given year.	NLSY79
Is Working Dummy (Weekly)	A dummy variable that equals one if the respondent was working that week and zero otherwise.	NLSY79
Log Work Hours (Weekly)	Natural logarithm of one plus total number of work hours in a given week.	
Log Wage Income (Annual)	Natural logarithm of one plus real wage income (in 2010 thousand dollars) in a given year.	NLSY79
Household Credit		
Log Mortgage Debt	Natural logarithm of one plus real mortgage debt (in 2010 thousand dollars) in a given year.	NLSY79
Log Car Loans	Natural logarithm of one plus real car loans (in 2010 thousand dollars) in a given year.	NLSY79
Log Other Debt > $500	Natural logarithm of one plus real other debt (in 2010 thousand dollars) greater than $500 in a given year.	NLSY79
Bank Account		
Has Savings Account	A dummy variable that equals one if the respondent's household has bank savings account.	NLSY79
Individual-Level Characteristics		
Education	Highest grade/years of schooling completed by the respondent, ranging from 0 (never attend school) to 20 (8th year college or more).	NLSY79
Age	The difference between the birth year and the survey year.	NLSY79
Family Income 7981	Natural logarithm of one plus the average total family income (in 2010 dollars) over 1979 to 1981.	NLSY79
Mother's Education	Maximum years of schooling received by the respondent's mother.	NLSY79
Father's Education	Maximum years of schooling received by the respondent's father.	NLSY79
D-Both Parents	A dummy variable that equals one if the respondent lived with both parents at the age 14.	NLSY79
AFQT Ability Test	Normalized percentile score after controlling for age groups among all the respondents in the 1980 Arm Forces Qualification Test (AFQT), which covers arithmetic reasoning, work knowledge, paragraph comprehension and numerical operations. Scores for the four sections were summed to form an aggregate AFQT score. A higher percentile indicates better learning aptitude.	NLSY79

Rosenberg Self-Esteem	An aggregate score ranging from 0 to 30 in the 1987 survey to measure the respondent's self-evaluation of personal worth. A higher value indicates a higher level of self-esteem.	NLSY79
Pearlin Sense of Control	A score ranging from 7 to 28 in the 1992 survey to measure the respondent's sense of control over one's life.	NLSY79
D-Risk Attitude	A dummy variable that equals one if the respondent answers "Yes" to the question "Would you take job that could either double family income or cut income by a third" in the 1993 survey, and zero otherwise.	NLSY79
State-Level Controls		
Per-capita GSP Growth	Growth of state-level real Gross State Product (GSP) per capita (in 2010 dollars).	BEA
Log Population	Natural logarithm of state-level population.	BLS
Poverty Rate	State-level poverty rate in percentage.	Census
Unemployment Rate	State-level unemployment rate in percentage.	BLS
Labor Force Participation Rate	State-level labor force participation rate in percentage.	BLS
Log House Price Index	Natural logarithm of state-level house price index.	FHFA
Small Business Loans		
Small Business Loans-Number	Total number of small business loans (in thousands).	CRA
Small Business Loans-Amount	Total dollar amount of small business loans (in millions).	CRA
Loan Number [<$100,000]	Number (in thousands) of small business loans less than $100,000.	CRA
Loan Amount [<$100,000]	Dollar amount (in millions) of small business loans less than $100,000.	CRA
Loan Number [$100,000, $250,000]	Number (in thousands) of small business loans between $100,000 and $250,000.	CRA
Loan Amount [$100,000, $250,000]	Dollar amount (in millions) of small business loans between $100,000 and $250,000.	CRA
Loan Number [$250,000, $1,000,000]	Number (in thousands) of small business loans between $250,000 and $1,000,000.	CRA
Loan Amount [$250,000, $1,000,000]	Dollar amount (in millions) of small business loans between $250,000 and $1,000,000.	CRA
Partitioning Variables		
Individual-Level Partitioning Variable		
Income Group	We calculate individual-level average real income (in 2010 dollars) over the three-year window before the sample period. The sample is divided into high- and low-income groups based on the median.	NLSY79/PSID